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idiom. Another objection is, that the practice introduces a distinction difficult to maintain on account of the graduation of the nominal into the adjective sense. 'The Carboniferous' may or may not imply some such noun as formation, and the degree of such implication is variable.

Authors of the second group speak of the Potsdam and Potsdam strata, but of the carboniferous and carboniferous strata. The distinction thus made is etymologic, being based on the immediate derivation of the name of the formation. To this there are two objections. First, it is contrary to the analogies of the language, for capitalization is generally controlled by meaning. We speak of 'the Pacific,' although the designation is etymologically a common noun; and we call the recently popular feminine waist-gear a jersey, although the designation is etymologically a proper noun. Second, it has the effect of recalling attention continually to the derivation of names, and thus retaining their connotative meaning. For mnemonic reasons, and for these only, it is convenient that names of formations should originally be connotative, but it is of prime importance that they should eventually become merely denotative. There was a certain original utility in having 'Potsdam' call to mind a place, and 'carboniferous' a character; but the names having become securely attached to their several formations, it is now imperatively demanded that each shall designate a certain portion of the stratigraphic column and a certain portion of geologic time, without connotating place or composition. Indeed, the reason why modern usage employs geographic terms in the naming of new formations, instead of designating them by their physical characters, is that a minimum of connotation is thus secured from the outset.

Authors of the third class capitalize all names of formations, whether used as nouns or adjectives, and in so doing escape these evils. The only objection I see to their practice is, that it classes with proper nouns a group of names which may fairly be compared with other groups not so classed. The demarcation between common and proper nouns is essentially somewhat obscure; and the drawing of the line is largely a matter of practical convenience. It is noteworthy that no author whatever has so drawn it as to include all names of formations with common nouns.

The capitalization of all formation names has the manifest advantage that it enables one to say that the Carboniferous rocks are not the only carboniferous rocks, or, in other words, that it does not deprive the geologist of the independent use of words indicative of rock character which have been appropriated for the names of formations. If the use of capitals were altogether discarded in the designation of formations, this advantage would be lost, but another would be gained; for we should then be able to speak of the rocks of Potsdam without implying their potsdam age.

G. K. GILBERT.

Remsen's 'Theoretical chemistry.'

Will you kindly allow me to correct an error into which it seems that I fell, in my notice of Professor Remsen's 'Theoretical chemistry' (*Science*, ii. 826)? It cannot be denied that the statement, "Of the substituting products of benzene which contain three substituting groups, more than three varieties have been observed," is literally true. The context and form of expression were such that I could not but think this assertion was made of those derivatives in which the three substituting groups were alike. Had it occurred to me that the statement was not thus lim-

ited, I certainly should not have pronounced it rash, but so cautious and incomplete that it must inevitably mislead even the most careful reader.

THE CRITIC.

Synchronism of geological formations.

I trust that you will permit me a little more space to reply to the further remarks of Mr. Nugent on this subject (*Science*, iii. 33), seeing that your correspondent has failed to grasp the point which I had intended to elucidate in my last communication.

Mr. Nugent is correct when he contends that I rest my case on the non-occurrence of 'evidences of inversions;' and, if my line of argument based on this fact fails to meet with his approval, I sincerely regret it. Paleontology, as far as I am aware, has thus far failed to show a single unequivocal case of faunal inversion such as I have indicated; nor does there appear at the present time very much likelihood of its ever being able to do so. Nor would the discovery of a solitary instance materially affect the question, inasmuch as, upon the theory of very broad contemporaneity suggested by Huxley, instances of inversion ought to be about as numerous as those of non-inversion. My courteous critic admits that "there is no reason why such instances of inversion should not have occurred over and over again," and that at the present time their 'occurrence is almost unknown;' but his appeal to the 'imperfection of the geological record' (both geological and geographical), in explanation of the overwhelming negative testimony, will, I am afraid, scarcely meet the situation.

The special cases referred to — Barrande's colonies, and the intermixture of Silurian and Devonian, and Devonian and carboniferous fossils in the old red sandstone of Scotland — are far from being of the character desired. The former need scarcely to be commented upon, since they have always been involved in a certain amount of obscurity; and their very existence as such has very recently been denied by Marr, who personally examined the region, Lapworth, and a host of other geologists. In the case of the old red sandstone of Arran, where there is an intercalation of a band of marine limestone containing *Productus giganteus*, *P. semireticulatus*, *P. punctatus*, *Chonetes hardrensis*, *Spirifera lineata*, and other well-known carboniferous fossils, Professor Geikie (who, we believe, first made the observation) distinctly affirms that these organisms must "have been in existence long before the formation of the thick Arran limestone," and that their habitat during the period of the deposition of the underlying sandstone was immediately outside of the basin or basins that through upheaval were now being gradually isolated from the sea: in other words, we have here merely an instance where the range of a certain number of organic forms has been extended somewhat lower down in the geological scale than it had hitherto been indicated. These same forms re-appear in the superimposed lower carboniferous limestones, and, as Professor Geikie observes, they must have been living during the long interval coincident with the sedimentation of the intervening sandstone 'outside of the upper old red sandstone area.' The same relation holds with the Siluro-Devonian mixture in the basal old red of Lanarkshire. No one can deny the local displacement and interchange of portions of two consecutive faunas, especially at about the beginning or close of their own respective series; but these displacements are not of the nature of the inversions that ought to illustrate the doctrine of broad contemporaneity.

To what extent similar or identical faunas indicate absolute chronological relationship can probably never

be determined; but I believe it may be safely assumed that the synchronism is defined within comparatively narrow limits; or, as previously expressed, "formations characterized by the same or very nearly related faunas in widely separated regions belong, in very moderate limits, to approximately the same actual age, and are to all intents and purposes synchronous or contemporaneous" (*Science*, No. 41). Professor Geikie, who is quoted by your correspondent as supporting the orthodox doctrine of homotaxis, or homotaxis in its broadest limits, judiciously refers to chronological divergences of only *thousands* of years, and *not* of *millions* ('Text-book of geology,' pp. 617-619).

ANGELO HEILPRIN.

Academy of natural sciences, Philadelphia,
Jan. 12, 1884.

Free cervical ribs in the human subject.

I send you a photograph of a notable and very interesting anatomical preparation well worthy of be-

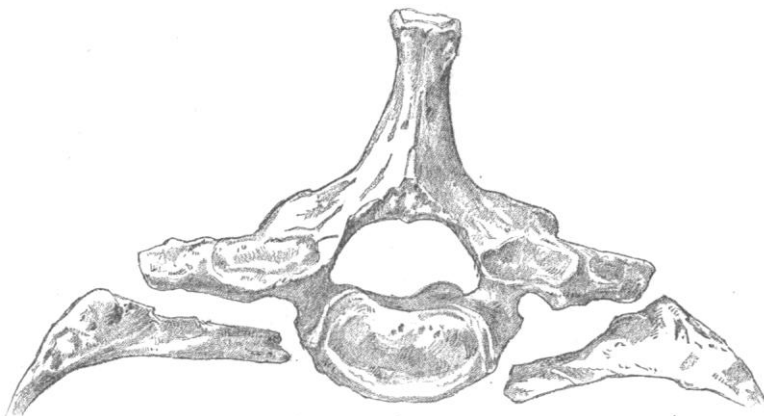
ing in possessing two demifacets, instead of a full facet above and a demi-one below. The same subject was also badly put together in some other respects; e.g., one of the long thoracic ribs (I think the fifth) bifurcated at the sternal end. The specimens were handed to me by one of my pupils, Mr. Arthur J. Hall. The anomaly here figured, while not new, is so rare that I think I have seen but one illustration of it; namely, that given by Professor Owen in his 'Comparative anatomy and physiology of vertebrates.'

ELLIOTT COUES.

Smithsonian institution, Washington,
Jan. 4, 1884.

A possible solution of the standard time question.

Although the adoption of five standards of time for the movement of railroad-trains in the United States has simplified the time question for the trav-



Seventh cervical vertebra of the human subject, life size, seen from above; showing well-developed and freely articulated pair of cervical ribs.

ing engraved and published in *Science*. It is the seventh cervical vertebra of the human subject, natural size, viewed from above, showing a pair of free cervical ribs. This demonstrates the fact that the so-called transverse process of a cervical vertebra consists of a diapophysis with a coalesced pleuropophysis, the vertebral foramen so characteristic of cervical vertebrae being an opening between these two apophyses. The photograph shows the preparation so well that little description is required. The whole bone is seen to be a little distorted, and the two ribs are seen to be of different shape and size. The ribs are photographed a little apart from their respective articulations, otherwise *in situ*. Each freely articulates, as usual with ribs, by its head with the body, and by its shoulder with the diapophysis, of the vertebra. The base of each diapophysis presents anteriorly a nick (deeper and more regular on the left than on the right side) which is a part of the vertebral foramen proper, the rest of which is circumscribed by the rib itself; the whole space between the vertebra and the neck of the rib being thus a large continuous opening of irregular contour.

The lower border of the body of this vertebra presents on each side a demifacet (not shown) for half of the head of the next (first dorsal) rib; so that the first dorsal vertebra must also have been anomalous

elling public, I believe it is a matter of deep regret, that, since a change has been made, that change could not have been to a single standard instead of five, and that Greenwich time, as Mr. Schott very significantly queries in *Science*, No. 38. This is the more to be regretted, since the railroad companies have found it impracticable to make the changes on the proposed meridians, and since, as Mr. Schott rightly apprehends, all ordinary business must always be conducted on local mean solar time.

It appears to me that this whole question could be very simply and forever settled by the adoption of Greenwich time for the movement of all public conveyances the world over, and the construction of time-pieces which would indicate at once both local mean solar time and Greenwich time. The only modification of the ordinary time-pieces needed, to enable them to indicate both times, is to provide them with two dials, one of which shall be movable about an axis, and capable of being set at any desired point. It is immaterial which dial is stationary: the same set of hands would sweep both dials, and indicate, of course, both times, at once. Thus provided, a person desiring to take the next train would be governed simply by the Greenwich dial. Furthermore, should his time-piece lose or gain, it would only be necessary to set it by either local mean solar time or by that of